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## ***Interactive comment on “Chlorine activation on stratospheric aerosols: uncertainties in parameterizations and surface area” by T. Wegner et al.***

**T. Wegner et al.**

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We thank the anonymous reviewer for his comments and suggestions. Please find a line by line response below.

Specific comments While overall I find the results very compelling, I wondered how sensitive the results for the Geophysica flight on 7 March 2005 are on possible temperature biases along the calculated trajectories. If I understand this correctly, only a mean bias, based on comparison with temperature measurements, was subtracted. If the bias is temperature dependent, how would that influence the results? For the flight

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on 7 March 2005 there is excellent agreement between measured and modeled ClOx before 0845 and after 0930, but between 0845 and 0930 modeled additional chlorine activation (difference between modeled ClOx and initialization) is underestimated by almost a factor of 2. How sensitive is this to the assumed temperature bias?

**We have subtracted a mean temperature bias only where temperatures along the trajectories are below 205 K. It is certainly feasible that a bias in the analysis data is temperature-dependent since our measurements indicate a larger bias at lower temperatures. Since heterogeneous reaction rates depend non-linearly on temperature, the modeled chlorine activation is highly sensitive on the assumed temperature bias. A larger bias at lower temperatures leads to more activation of chlorine in our study and increased agreement between 0845 and 0930. However, a larger temperature bias is not supported by our measurements.**

And how large is the uncertainty for the inferred initial ClOx, given that this is calculated as the difference between inferred Clx from CH4 and satellite observations of HCl and ClONO2? Providing a more quantitative estimate for the uncertainties here would strengthen the conclusions of the manuscript.

**We have added a section describing the uncertainty of the various measurements/correlations and how their influence on uncertainty of modeling chlorine chemistry along the trajectories.**

To me the sub-title “uncertainties in parameterizations and surface area” does not correctly represent the main findings of this study as there is little new material that tells us about uncertainties in parameterizations.

**The title has been changed to: “Heterogeneous chlorine activation on stratospheric aerosols and clouds in the Arctic polar vortex”**

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Why are the parameterizations by Hanson and Ravishankara and by Abbatt and Molina (p.20567) considered as an upper and lower limit, respectively? Simply because they represent the range of available parameterizations, or are there any deeper reasons to assume they constitute limits?

**These two parameterizations constitute the fastest and slowest published published parameterizations for NAT reactivity. There is no reason to prefer one over the other and it is yet unknown which one gives a better representation of heterogeneous chemistry on NAT.**

Minor comments and technical corrections p.20564, l.18: This sentence sounds a bit as if “deliberate enhancement of stratospheric aerosol” is imminent.

**rephrased**

p.20566, l.9: Please explain briefly how the almost complete reduction in ozone leads to a Cl increase. (Through the ozone-dependent partitioning between Cl and ClO.)

**added**

p.20566, l.16: don't link deactivation of Cl into HCl and almost complete ozone destruction by “and” in a single sentences, as this implies the wrong causality.

**rephrased**

p.20566, l.22: suggestion: “which describes” -> “describing”

**corrected**

p.20567, l.10: better discuss the dependence on H<sub>2</sub>SO<sub>4</sub> and temperature separately

(not linked in a single sentence by “and”).

**corrected**

p.20567, l.14-18: better move these lines somewhere into the next paragraph.

**moved**

p.20568, l.12: say explicitly which ClOx compounds are measured (ClO and Cl<sub>2</sub>O<sub>2</sub>).

**corrected**

p.20568, l.12: “...and back-trajectories indicate that chlorine was activated...”: there is some circular reasoning here. Either say "...temperatures along back-trajectories indicate that chlorine activation was unlikely before..." or remove this sentence here.

**rephrased**

p.20569, l.4: symbols theta and phi not needed

**removed**

p.20569, l.22: why are the findings of Brakebusch with WACCM relevant here? Is the 1.5K temperature bias more than just a pure coincidence?

**The study of Brakebusch et al. is mentioned since it covers the same Arctic winter and finds a similar temperature bias. Since they use independent datasets (SD-WACCM in Brakebusch et al. is nudged with GEOS-5 data) it appears unlikely that the temperature bias is pure coincidence or an interpolation artifact. However, it is beyond the scope of this work to validate polar temperatures in**

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reanalysis data.

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12, C9311–C9316, 2012

p.20569: how is the temperature bias as a function of potential temperature calculated (Fig.2)? Is ERA-Interim mapped to the Geophysica altitude using potential temperature, or using only pressure? Calculated temperature bias depends on this detail.

**clarified. ERA-INTERIM data is first interpolated on isentropic coordinates and temperature is subsequently linearly interpolated in space and time on the measurement positions of the TDC.**

p.20570, l.3: “greatest” -> “largest”

**corrected to “maximum of the warm bias”**

p.20573, l.7: how different are these cross sections with those from the latest JPL recommendations?

**The scaled von Hobe cross section is smaller than the current JPL recommendation but the choice of cross section does not significantly affect our results. A detailed comparison of the different cross sections is given in Suminska et al. (2012)**

p.20574, l.16: “In 2004, ...” -> “... steep decline...at the end of December 2004 and at the end of December 2009”.

**corrected**

p.20577, l.22: “could demonstrate” -> “demonstrate”

**corrected**

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p.20577, l.23: "last seven": please specify explicitly Captions to Figs. 2-4: Please include date of flight.

**corrected**

Interactive comment on *Atmos. Chem. Phys. Discuss.*, 12, 20561, 2012.

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